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Presidential Address

AT THE

EPIDEMIOLOGICAL SOCIETY,

Delivered on November 5th, 1879,

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GENTLEMEN.—My first duty is to thank you for placing me in the honourable and responsible position I now occupy as President of this Society. I must suppose, whatever may be my own misgivings on the subject, that you recognise in me some personal or official fitness for the post, and it therefore only remains for me to endeavour to justify the selection you have made, though I confess I anticipate some difficulty in so doing. The duties of a Society with such aims as those of the one over which you have called on me to preside are of a very important nature; the questions it proposes to investigate are often of surpassing interest, and the influence it ought to exercise for the public good should be largely exerted and widely felt. To guide the affairs of such an institution seems to me to demand, in the person who holds the office of president, a combination of qualifications that I can lay no claim to: nevertheless, as you have been pleased to place me here, it will be my duty and my anxious desire to devote to your service such knowledge as I may have acquired during a long residence in India, where epidemic disease prevails; and for the rest, I can only crave your indulgence for such shortcomings as must occur in the case of one whose experience has been rather that of a practical physician than of a scientific epidemiologist; assuring you, at the same time, that no effort shall be wanting to promote the welfare of the Society.

On assuming this seat, I know it is my duty to inaugurate the session by a brief address on some subject cognate to the work before the Society, and I must therefore observe the usual routine; but I admit I approach it with some hesitation, for there is so much that might be said that I hardly know what to select as most appropriate to the occasion. I trust, however, that I shall not greatly err in selecting a subject connected with the Progress of Epidemiology in that great country which contributes to the British Empire 250,000,000 inhabitants, composed of many races; and territory as large as all Europe, excluding Russia—including every climate from that of the arctic to the torrid zone; and that is regarded by many as the birthplace and permanent home of the most terrible epidemic disease to which the human race is liable.

We are now entering on our thirtieth session, and, I trust, with reason to believe that our progress is satisfactory, and our work such as to prove that the Society is fulfilling the main object for which it was founded. Though not one of the largest, it is certainly not one of the least active or important among the medical societies of the metropolis, whilst the cosmopolitan range of subjects embraced within the scope of its inquiry renders its proceedings of far more than mere local interest.

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The papers read and subjects discussed during previous years vindicate its title to a prominent place among institutions which have for their object the promotion of some of the best interests of mankind; and I can only express a hope that the session now commencing will not fall short, in this respect, of its predecessors.

As a comparatively new member of the Society, I have as yet had but little experience of its mode of working, and few opportunities of taking much part in its discussions, but I have learned enough of its objects and proceedings to know that they merit the best effort of the members for its support, and also for the elucidation of the important (and, in some cases, still undetermined) questions that come before it, and which so deeply concern the welfare of man and the prosperity of nations—problems relating to the genesis, diffusion, and prophylaxis of disease which, in the epidemic form, still so often prove the scourge of communities and the enigma of science. Such are the subjects it is our duty to investigate, and by degrees we hope in some measure to unravel. Nor need we doubt that in time much of what is now obscure will be made clear. When we think of what has been done but recently—how much has been learned by careful observation and generalisation, how beneficial has been the result of the knowledge as applied to sanitary work, and in the diminution it has effected in the death-rate, and when we contrast our present knowledge of idiopathic fevers and other diseases, the laws of contagion, and the natural history of entozoa, with that of but a few years ago, we may reasonably hope that the work of such a Society as this will be prolific of good results, aided, as I trust it always will be, by the experience of men who have studied disease, not only by the bedside of metropolitan hospitals, but in all climates in every quarter of the globe, in the army, navy, colonies, and in our Indian empire (which may be regarded as the head-quarters not only of epidemic, but also of some special form of indigenous or endemic disease). Regarding it from this point of view we naturally desire to have a large accession of members on our rolls, and to see a greater share of the interest that is so freely bestowed on the pathology and therapeutics of special forms of disease accorded to epidemiology, to disease in the aggregate, or as it affects the masses of the population, when, in obedience to laws with which we are as yet but imperfectly acquainted, it moves in and from city to city, over rivers and across seas, from province to province, from continent to continent, until at length it disappears apparently as mysteriously as it arose, though, no doubt, in conformity with constant and definite laws. No one can have watched the rise, progress, and decline of an epidemic of cholera or of fever, such as the dengue that last swept over India in 1872, with their apparent caprices of invasion and subsidence, activity and decline, without feeling how much he has still to learn before he can comprehend the import or reason of it all, and how essential it is that we should study, with all the aids that science can bring to bear on the inquiry, the complex and recondite agencies or forces that control the origin and movement of epidemic disease; patiently studying its various phases and phenomena, carefully substantiating facts, not allowing ourselves to be unduly influenced by the bias of preconceived theories, and endeavouring to record these facts *as they are*, and not *as how we think they ought to be*; for thus shall we gradually accumulate

reliable data from which to generalise, and on which principles may be based and laws established. Nor is such caution superfluous, as I think will be admitted by some who have had to deal with sanitary reports.

The *raison d'être* of this Society is the investigation and development of our knowledge of disease in motion. It involves much, for any disease where it spreads, whether among the people of a house, a ship, a village, a city, a province, or a continent, is an epidemic, and comes within the scope of our inquiry. Dysentery and malarious fevers are typically *endemic* diseases, but in India they may and do at times assume a dangerously epidemic character. But it is not meant that our inquiries should be restricted to mere epidemicity alone; we cannot advantageously study one phase of the natural history of disease and exclude others. There is so much in etiology, semeiology, and pathology both human and comparative, that concerns our department of research that we may not ignore the means by which we gain the most important of all information to the epidemiologist—namely, the means of discriminating one form of disease from another. Therefore, though our proceedings will naturally refer mainly to epidemic disease, we shall thankfully receive and carefully consider all information that may tend in any way to throw light on the causal relations, and on the influences exercised by climate, season, locality, food, and occupation on the genesis and dissemination of all diseases, not excluding those of the lower animals, or even of plant life. I might illustrate this by referring to the importance of discriminating between the different forms of fever that occur in India. It is well known that the greatest proportion of mortality in India is ascribed to fevers. The highest death-rate¹ was registered in Bombay, where it equalled 20.82 per 1000; in Madras, the deaths of 469,241 persons gave a death-rate of 10.08 per 1000. Both here and at Bombay, however, the mortality recorded was greatly aggravated by famine. The statistics of "fevers," in all years, are admitted to be most inaccurate, but the deaths registered under this head in the different provinces during the year 1877 were as follow:—

Bengal	711,037
North-west Provinces	574,722
Punjab	219,281
Central Provinces	131,123
Berar	34,453
British Burmah	261,001
Madras	469,241
Bombay	336,865
Assam	18,725
					2,756,448

In 1878 there died in Bengal, of diseases classed as fevers, 742,887 persons (419,294 males, 323,593 females). The total death-rate from these diseases was 12.38 per 1000, against 7.58 per 1000 average on the previous five years. Part of this may be attributed to better registration.

In 1877, out of a population in nine provinces of India of 181,929,889, there died of fevers 2,521,438; whilst of cholera 635,977 died. In 1874 only 81,266 died.

¹ Vide Report of Sanitary Commission of the Government of India for 1877.

In his report for 1878 the sanitary commissioner, Dr. Little, of the Hyderabad assigned districts, shows that there was exceptional mortality during that year. In a population of about 2,186,988 the death-rate was $81\frac{1}{2}$ per 1000 in 1878, against 32·1 per 1000 in the previous year. The total deaths in the year were 178,404, and the causes were as follow, fever being the most prominent :—

Death causes.	Total deaths.	Deaths per 1000.
Cholera	34,306	15·6
Small-pox	5,850	2·7
Fever	85,260	38·9
Bowel complaints	27,577	12·6
Injuries	1,1475
All other causes	24,264	11·1
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All causes	178,404	81·4

This great mortality is probably partly attributable to the indirect influence of the famine, which deprived the province of much of its most wholesome food by causing exportation of grain to the famine districts. The population to a great extent thus subsisted on an inferior, or deteriorated, kind of grain. There were other local climatic causes, but to these I need not allude. I refer to the subject as an evidence of the great mortality of fevers as compared with other diseases.

There is no doubt that, under the heading Fever, many deaths from other causes are recorded, and we may probably refer a large proportion of them to diseases of an inflammatory character affecting the thoracic or other viscera, or to complications involving inflammatory action elsewhere. In a vast country like India, where the population is so extensive, and the means of registration of necessity limited, often not under medical supervision at all, it is not to be expected that greater accuracy can be ensured ; but, were it possible to discriminate among the various forms of disease returned as causing the mortality by fever, we should have a very different result from the present. One can hardly refer to this subject without expressing admiration at the great progress that has been made of late years in registration under the direction of the sanitary authorities of India, and confidence that it will continue to improve, and render the statistics more valuable even than they are now. Of course, where the registration of death is not subject to medical definition, discrimination between the various forms of fever or other death-causes could hardly be expected ; and therefore the example I have just given is hardly so good an illustration of what I refer to as typhoid, for which we have accurate medical statistics of our European troops in India. It is within my recollection that attention was first called to the existence of this form of fever in India, and yet there can be, I suppose, no doubt that it has always been there. It soon became generally recognised as a new discovery in India, and people wondered how it had escaped observation hitherto, whilst some perchance regarded it as a new disease. But it was just this power of discriminating observation that is so rare and so valuable that had been wanting ; it was this that, exercised by Budd, Jenner, Murchison, and others after them, established a new era in the nosology of fevers in England ; and it was this that, a little later, in India, discriminated between certain forms of remittent and enteric (*i.e.*, between

malarious and specific) fevers, and that will, I hope, ere long further discriminate and rearrange the nosology of fevers in India and the tropics.

Now typhoid or enteric fever is an important cause of mortality among our young European soldiers in India; and it raises or suggests questions of great importance in regard to them—such, for example, as the right age, time, and seasons for sending them to India; to say nothing of the hygienic questions as to sanitary measures arising out of its causal relations. The Sanitary Commissioner's Report of 1877 says that out of 233 cases of typhoid, 92, or 39 per cent., proved fatal; the admission rate being 4·1 per 1000 of strength. It moreover appears that 2·45 per cent. occurred at or under twenty-four years of age; 1·55 at twenty-five to twenty-nine; 0·99 at thirty to thirty-four; and a few or none above that age; showing that the disease tells most severely on the younger men—in this respect resembling typhoid in England. Again, Bryden, in his Report of the Statistical History of the European Army in India up to 1876 (published 1878), says: “It has no geography; and it is a matter of popular observation that no regiment or battery escapes enteric fever *in the first year*, whatever cantonment of India may be selected.” “Out of seventy-three bodies of men two regiments and seven batteries only returned no case of enteric fever in the first year.” And he gives the following analysis of 368 deaths that occurred between 1823 and 1876:—

Ages.	Total deaths.
24 and under	255
25 to 29	90
30 to 34	17
35 to 39	4
40 and upwards..	2

Seventy-five of these deaths occurred within three years after landing in India, and 94 per cent. of the total were among men under thirty years of age. In a memorandum received only a few days ago Bryden says, out of 132 deaths from enteric fever in 1878, 90 occurred in men who had been under twenty-two months in India. All this shows that youth and the first year of service in India are the great predisposing causes.

Now is this the same disease as that which might be contracted in London, Dublin, Windsor, or elsewhere in a town or barrack?—from a watercloset, drain, sewer, well, or, it may be from a milk-can? I have little doubt that very frequently it is exactly identical; but I believe also that perhaps as frequently, or more so, it is not; and this, I believe, not in regard to young European soldiers only, but of the whole population of India. In short, I am, and long have been, of opinion that a form of fever exactly like European typhoid, except in its etiology, exists in India and other hot and malarious countries; and that it is due to climatic causes, not to filth or specific causes such as give rise to it in England and elsewhere, and recent reports from India and other parts of the world seem to show that this view is gaining ground. I am indebted to Surgeon-Major A. Clark for the following note:—

“Typhoid fever has prevailed very extensively in Natal and Zululand during the war. Since Jan. 1st, 1879, to May 31st, no less than 267 admissions for enteric fever have been recorded. Many of these occurred in healthy camps on ground previously unoccupied (virgin soil), and in bodies of picked men. The water supply, as a rule, good; no

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sewers or drains; conservancy, dry earth or trench, and carefully attended to. The troops of Crealock's Division, which were encamped near the coast and in close proximity to marshy ground, where the natives suffer severely from remittent fevers, had more admissions from enteric than the Second Division, which was inland and generally at higher elevations. The medical officers differed as to this fever, several maintaining it was not typhoid as commonly understood in England, but bilious remittent with typhoid symptoms, as seen in India; others nailed their colours to the mast that it was genuine enteric. In support of the former, a medical officer just home reports that numerous cases entered as enteric were discharged, and at their duty five or six days afterwards. Surgeon-General Woolfreyes describes this fever as 'typho-malarial.' He says: 'It is not a fatal fever, but it causes great prostration, and a change to England is absolutely necessary. I am of opinion that it is climatic, the true autumnal fever. It, as a rule, commences with sore-throat, a peculiarity; the rose spots are invariably present, and in fatal cases the lesions of Peyer's patches are well marked.' At the time that enteric was reported so common in the First Division, jaundice also prevailed very extensively, but was unknown in the Second Division. Enteric has also been reported as causing much sickness amongst the troops in Afghanistan. Here, again, camps were often pitched on virgin soil, though the water supply was far from satisfactory, and dead camels so constantly polluted the streams. It prevailed *with* cholera and severe remittent fevers. Altitude made no difference, cases being admitted in camps several thousand feet above sea-level. Enteric cases are reported from nearly every station in the Bengal Presidency, some such as Cambellpore, in the Punjab, where the 'filth' element is at a minimum. At this station there is no large bazaar or city in the vicinity; the soil is arid, sandy, and very dry; the rainfall exceedingly small; water-supply good, and very carefully filtered and attended to; the dry-earth conservancy is carried out to perfection; the milk danger is almost *nil*: yet enteric occurs, and amongst the troops who drink hardly any milk, while the women and children who probably drink a good deal of bazaar milk have so far escaped. These facts seem to show that enteric fever cannot be always ascribed to a 'filth' cause, but that climate, or what is embraced in that unknown word, 'malaria,' may give rise to it as it undoubtedly does to intermittents, remittents, and possibly cholera."

Be it clearly understood, however, that I do not for a moment dispute the existence of genuine *filth* typhoid in India. The official returns, which cannot be gainsaid, and my own experience alike leave me in no doubt about it. But I do believe that many cases now recorded, and rightly recorded, as typhoid in India, are not caused by the same specific agency as that which gives rise to typhoid here, and also in India, where the necessary conditions are present. That there are, in fact, two, perhaps more, forms of typhoid, or perhaps, I should say, different causes for a disease presenting the same phenomena, one being specific, the other climatic; if so it is obviously very desirable to discriminate between them—not merely as a matter of nosological or pathological interest, but in regard to the hygienic measures that are necessary in either case. This question is of great interest to epidemiologists, and

is just one of those subjects that should come within the scope of our inquiry. A remarkable instance occurred near London lately, where an *epidemic* of typhoid was traced to its origin along the track of a particular water-supply by a distinguished member of our Society. Our lamented colleague, Murchison, also traced a similar outbreak to contamination of milk by polluted water. The explanations of these outbreaks were exactly in accordance with these views, no doubt, rightly held in Europe. Typhoid in India, however, would not always be similarly explained.

Questions of this nature are, I think, such as might be advantageously discussed here. I am aware that great differences of opinion exist among those whose views command the highest respect; now, to compare ideas, to discuss, and, it may be, to reconcile differences, to clear up doubts for ourselves and others, to confirm or refute, seems to me to be our legitimate work. Our views on a particular subject may be unsettled; they may be influenced by preconceived theory, and altogether far from confirmed. One can understand that a cautious and reflecting mind may desire more evidence before accepting either view presented as the correct one, or before admitting that it has been convinced. In regard to some questions of etiology such, I believe, is far from an uncommon frame of mind. The consideration of such vexed questions is most desirable. Let us hope that free expression of opinion, and temperate discussion of views, however opposite, will here be attended by good results.

The late Mr. Bedford, whose comparatively early death was deplored by the Bengal Medical Service, of which he was a brilliant ornament, said in a letter to the President of the Society in the year 1850 : "Epidemic diffusion is the most important medical question of the day, and can only be studied through systematic and parallel observations conducted by a number of intelligent men working under one head. India, from its climatic peculiarities and the nature of the service, offers, I hesitate not to affirm, the most magnificent field in the world for carrying out such a series of connected observations as may tend to elucidate the laws of the most terrible disease which flesh is heir to, and it will redound to her eternal honour to be in advance of such inquiries." Whether these aspirations have in any way been fulfilled will appear from what I have to say; it will, I think, be seen that India has contributed in no small degree towards the attainment of the end so much desired; and that we owe much to the labours of those distinguished men whose writings, published in monographs, in reports, and official returns, afford ample proof how earnestly they have laboured to advance our knowledge of disease. The names of Jameson, Scott, Annesley, Twining, Martin, Mackinnon, Morehead in earlier times; of Goodeve, Chevers, Mouat, M'Lean, Bedford, Ewart, Bryden, Murray (our late distinguished president), the Cunninghams, Payne, Lewis, Macpherson, Cornish, Macnamara, Hewlatt, Townsend, Coates, Planck, and many others subsequently, to say nothing of the numerous valuable medico-topographical reports by the medical service generally, represent a literature of epidemiology that is unsurpassed in richness of its material, and affords evidence that the subject has received and is receiving elucidation that bids fair in time to throw such a flood of light on the natural history of epidemics as will reduce our knowledge to a state of exactitude hitherto unknown. They have, it is true, taught us

but little of the nature of the causes of epidemics such as cholera, or of malarious diseases, but they show what may be expected in regard to them, when and how they will appear, and how sanitary work may be made most effectually to avert, prevent, control, mitigate, or remove them; nor is it too much to say that we hope, if not to banish or stamp out, at all events still further to mitigate their ravages. This, I think, is warranted by experience, for certainly the past twenty years have witnessed great progress, and we already see that not only is the value of life increasing, but that the virulence of, and mortality caused by, epidemic disease are being controlled, whilst the vexatious and purposeless restrictions and restraints of quarantine are diminished if not altogether removed, in our dominions at least. Science that has enabled us to reduce the death-rate among our troops from 17·9 to 8·56 per 1000 in Europe, and from 69· to 17·62 per 1000 in India, speaks for itself; and were there no other result this alone is a triumph such as has been achieved by no other department of knowledge. Pray do not suppose that I claim all this for Indian workers. We all know that these great steps in social and sanitary progress began here, but I do say that the torch then lighted has since been worthily and firmly borne in Indian hands.

This Society, young as it is, can remember the commencement of systematic sanitary work in the East, and may claim some share in the origination of the good work; for among its earlier members were some who advocated the study of epidemiology and hygiene in India. I cannot now stay to dwell on this, but I may say briefly in regard to its progress and work that until the sanitary department was formed, less than twenty years ago, comparative, I might almost say complete, ignorance on the subject of epidemics, and of the diseases that prevailed among the people, existed. An epidemic might carry off thousands, but we knew not where it commenced, where it ended, or what area it occupied. Now, thanks to the continued and careful statistics, we know all that and more with fair accuracy, and are gradually collecting facts which make the study of epidemiology possible. Before organised sanitary work in India began our knowledge of the general population—nay, even of the European troops and prisoners—was most imperfect. Now, thanks to that department, and especially to Bryden, whose name cannot be too prominently mentioned in connection with the subject, we have, in his most elaborate and valuable statistical reports, facts and figures, as well as deductions, which deserve the closest attention. We have, in short, the most complete details of sickness and mortality in all classes over the whole of India. It is impossible, however much anyone may differ from his conclusions, not to recognise the great value of his work, for these reports contain a vast and continued array of authenticated facts which will serve as a mine of information to epidemiologists; and there is every reason for believing that it is but an earnest of more; for if, as Bedford said, the circumstances of India were such as to favour the acquisition of knowledge in 1850, when the precise conditions of life of the population generally, and even of our troops and prisoners were imperfectly known, what must it be now when a system of observation, carried out by a body of trained observers under a head such as he contemplated, is in full and daily improving operation? One can only wish that, considering the magnitude of the work, it were more

extensive still, and that observations, already of the greatest value, could be rendered still more so by being concentrated on certain limited areas so as to enable the inquiry to extend to details with a precision that at present can hardly be practicable. The results of epidemiological knowledge and sanitary work are seen in the effect already produced in reducing the mortality from cholera and other epidemics, and from malarial fevers. For instance, among our European troops, the circumstances of which are well known, there has been the following alteration in the general death-rate:—

1861 to 1865	9.02 per 1000.
1865 to 1870	6.98 "
1870 to 1875	3.23 "
1875 to 1876	2.3 "
1876 to 187784 "

And it can be shown from the same source (Bryden) that there is a similar reduction in the death-rate among native troops; and that, during the great cholera epidemic that prevailed in 1876, the death-rate among the civil population being 12.12 per 1000 (it was the famine year) that of the European troops was 1.75 and of the native army 2.2 per 1000. Also that in the death-rate of that great scourge of India—fever—there has been an equally happy result, as shown by the death-rate of a period of nine years—from 1868 to 1877—in the Bengal and N.W. Provinces gaols, as against a mean, in nine years ending 1867, of 22.41.²

	Per 1000.		Per 1000.
1859 13.76	13.76	1868 2.84	2.84
1860 49.19	49.19	1869 4.57	4.57
1861 38.14	38.14	1870 6.20	6.20
1862 30.81	30.81	1871 5.81	5.81
1863 25.44	25.44	1872 1.92	1.92
1864 29.96	29.96	1873 1.56	1.56
1865 7.65	7.65	1874 2.67	2.67
1866 5.23	5.23	1875 3.50	3.50
1867 3.12	3.12	1876 1.26	1.26
Mean .. 22.41	22.41	Mean .. 3.29	3.29

In India, as elsewhere, the purpose of epidemiological study is to observe accurately and to interpret the import of the facts—i.e., if possible, to elucidate the laws of which they are the expression,—and thus to form a scientific basis on which to direct sanitary work, which itself is the practical outcome of such observation, and concerns itself but little with theories. Those who have to do with it know how difficult it sometimes is to obtain *reliable* observations, such is the difficulty of excluding the bias of preconceived theory of the nature of the subject under investigation. Our Society, however, must hold the balance, and deal with theories as well as facts. No doubt the explanations sometimes are conflicting. Happily, in the practical mode of dealing with the question, there is not much conflict, and it is remarkable how little different is the action of those who hold opposite opinions on the causation of disease. As to the different views that are held in

² Bryden's Report (1876), p. 157.

regard to fundamental questions regarding the genesis and diffusion of disease let us hope that we may, perhaps, here contribute something towards their adjustment.

In illustration of the state and progress of epidemiology in India I might select the history of any of the great epidemics that have occurred of late years, but it would be impossible, in the short space of time at my disposal, to do this completely. I shall, therefore, confine myself to a few remarks on cholera, as it, though far from being the most destructive, is the epidemic to which most interest attaches. No disease has been more carefully studied, and the means of doing so exist nowhere so abundantly as in India, for whether in the sporadic, endemic, or epidemic form, it is seldom absent from some part or other of the empire. In the so-called endemic area in Bengal the opportunity of studying it is never altogether absent, whether it be sporadically or moving as an epidemic within the limits of the endemic area (its permanent home), or in the periodic outbursts, when it passes over the continent generally in what seems a capricious manner, but according to what really, as shown by Bryden, is the operation of laws that are now beginning to be well understood. The mode of invasion and diffusion, the rise and decline, the influence of locality, season, meteorology, conditions of soil, air, water, food, are all now closely studied, and to them various degrees of importance as factors have been assigned by different observers; and from the statistics conclusions have been drawn by Bryden and others that point with considerable force to the nature and the constancy of certain laws governing the progress of the disease; and albeit they have not as yet resulted in any very striking discovery, they seem to show that climatic and local phenomena form an important basis of a useful knowledge of the etiology of cholera, whilst the researches of Lewis and Cunningham point to certain conditions of soil as determining its production and development, as being most in accordance with the phenomena of its seasonal prevalence in the endemic area. But their prolonged and careful researches have as yet thrown no new light on the nature of the cause itself. Though it is only recently that this numerical method of dealing with epidemics has obtained in India, it is to be remembered that much valuable information has been recorded in times past in the writings of those to whom I have already referred and others, and in the records of the Medical Boards of the three presidencies, and reports of the sanitary commissioners. Indeed, since the advent of the Portuguese in the sixteenth century it has been described so graphically that one can hardly doubt the identity of the disease mentioned in early writings with that of the present day; in fact, it has as well defined a history as any other event of the past, and seems to have manifested the same phenomena, and to have been subjected to the same laws then as at present.

The theory that cholera is purely of Indian origin, and that wherever it may appear it is to be traced back ultimately to the delta of the Ganges, is disputed by some who see in history evidence that it has long existed in other parts of the world, and that it was described by the earliest writers—Sanscrit, Greek, and Arabian. I may here just remark that the name "haida," or "haiza," used by Rhazes nearly 1000 years ago in describing the symptoms of cholera is the same as that applied to it now by every Hindustanee-speaking native of India. As to its presence in

India from earlier periods we have descriptions by Correa, d'Orta, Bontius, and others, beginning from 1503. An epidemic of it in and about Goa in 1543, for example, is described by d'Orta, giving all the characteristics that distinguish an epidemic now. He calls it "moryxy" and also "haiza." It is described by a continuous chain of writers as occurring in various parts of India, in the interior as well as on the coast, up to the seventeenth century, when, after being quiescent during the latter part of the eighteenth and early part of the nineteenth century, it broke out with great virulence in Bengal, and has remained there ever since, in what Bryden calls its endemic area, whence it spreads according to certain laws, which are being worked out with admirable patience and intelligence by this distinguished statistician. I cannot now trace the history of cholera in either East or West, nor refer even to the numerous authors who have described it. For full particulars on this subject I refer you to Inspector-General Dr. J. Macpherson's learned work "The Annals of Cholera."

I need hardly insist that cholera is not a new disease, or that it did not, as supposed by some, make its first appearance as an epidemic in Jessore, in Bengal, in 1813; though no doubt since that period it has been more closely investigated and described. All seems to show that it is the same now as formerly, and that though we have gained much knowledge of its natural history of late years, yet we are as ignorant as our predecessors of its real nature. We have, thanks to sanitary measures, disarmed it of some of its terrors, and have diminished the mortality it caused; but as to treatment we have gained but little, though the empiricism of to-day is more scientific than it was in former days. We do not now burn our patients on the soles of the feet, tie ligatures round their limbs, or have recourse to other senseless barbarities; for we find that simpler and more rational methods are of greater avail, more or less according to the period of the epidemic at, and the promptitude with, which the remedies are applied. But we have learned that local causes have a potent influence, and that cleanliness, good air, *pure water*, and free ventilation are all powerful opponents of cholera; that we can predict its appearance and avoid it in certain places; and that it is not to be controlled by quarantine or sanitary cordons. And, from the earnestness and intelligence with which the subject of its etiology is pursued, it is not improbable that sooner or later it too will be made out. We shall then be in a position to say, not only what it does or will do, but what it *is*. Meanwhile we must go on observing and investigating. It is satisfactory to know that we are daily learning, practically, better how to deal with it, and how to modify its cyclical intensity and avoid its ravages. For my own part, until I know something more of the *nature* of the cause—be it a material poison, aerial or telluric, a miasm, or a dynamic agency that so perturbs the vital energy, I cannot see my way to formulate a definite theory either of the nature of its origin or the method of its diffusion. I find the highest authorities at complete variance on the subject, and reposing faith in theories diametrically opposed to each other. Some explain all the phenomena by contagion—*i.e.*, communicability in some way of a *materies* or germ from one person to another. Cholera, they say, is the result of infection by a poison derived from the intestines, and water or air, but especially water, is the channel through, by, or in which the infective

material is intensified and conveyed. These arguments are supported by an abundant array of facts, and have been maintained by men whose very names carry conviction. Others reject altogether this explanation ; they insist that local influences are all-important, and deny that the spread of cholera is due to human intercourse, that there is any poison transmitted by the excreta, or that the disease is in any way communicable from one person to another. They admit the existence of a poison of some sort—a miasm or an influence, though of its nature they are ignorant. It is a subtle thing that travels in certain directions in obedience to certain laws, is influenced by atmospheric and telluric conditions, and where it finds certain local conditions, and the people prepared by them to submit to it, there cholera will prevail. They deny the efficacy of any enteric or specific poison in the water to produce it, though they attach the greatest importance to the purity of water from *all* organic contamination, impure water being one of the local conditions which, if added to crowding, filth, or other insanitary conditions and want of proper ventilation, is that of *all others* which favours cholera. Such are the principal theories of the disease as they are supported by fact and argument which appear convincing. Europeans, Americans, and some authorities in India maintain the contagious view, whilst high authorities in India adhere to the opposite theory, and declare roundly that the facts of cholera, in India at least, are altogether opposed to the contagion theory.

Dr. Coates, in his last report on Bengal for 1878, says : “ Much has been written and much discussion has taken place during the year regarding the connexion between cholera and foul water. We have not yet discovered the poison of cholera, nor even if there be one. To limit the cause of cholera to one factor, and its entrance into the system to one mode only, would be very unwise. Short of demonstration, the objections against such inferences must be great. Notwithstanding this, I confess that every year’s observation, in going from district to district, in hearing the various opinions of the local officers on the subject, and in reflecting on both, my conviction has become stronger and stronger that there is a connexion between impure water and cholera, and the one—in Bengal, at least—is the chief (I would not make it absolute or conclusive) exciting cause of the other. Wherever the drinking-water has been most perfectly kept free from impurities, and especially local ones, there cholera has prevailed the least.”

I have no intention now of entering into the controversy, and have merely referred to the main points of difference that exist among those who, having watched the disease closely and critically, are equally entitled to be heard. So far as I can see no theory yet propounded meets all the difficulties, or explains all the phenomena manifested by cholera, at least in India. There is much in each that commends it to acceptance, though each is beset by difficulties. So far as I can judge the direct contagion theory has now few supporters anywhere, for few believe that mere contact with the sick is attended with danger, and many believe the same with regard to the excreta, the enteric contamination of water—the water theory, in fact. This is not the time or occasion on which to offer an opinion on the matter ; but I must say that, as far as my experience goes (and it has been pretty extensive), it inclines me to look for the explanation in wider and more general causes than those of infected water only.

I feel unable to declare myself an unqualified adherent of any theory yet propounded; the time, I venture to think, for dogmatising has not yet come. We may, I believe, neither accept nor reject altogether the contagion theory. I doubt if anyone can feel so confident as to say that cholera is altogether independent of contagion or, in some sort, of human intercourse for its diffusion; or that it is not so imported or conveyed to other localities. It is possible that it may play a part, though a subordinate one, in the transmission of the disease; and yet I apprehend one must look for other and more general causes to account for all the phenomena. It must be borne in mind also that there may be influences and forces at work of which we know nothing, and that cholera may have different modes of working in India from those in Europe. It is so with other epidemics. Yellow fever is subject to certain laws within certain parallels of latitude, or rather isothermal lines, but beyond these it is extinguished. Scarlatina can barely exist in India; it dies almost as soon as it is born. Cholera thrives for a time in other climates and countries, but perhaps under other conditions than those of India, and does not endure. That may be signally true of it in India which is only partially so in other climates, and the laws which regulate its diffusion may not be—probably *are not*—alike in all countries where it has occurred. We are hardly in a position yet to act on any theory that has reference only to its origin. Strangely enough the two most opposed in nature as working hypotheses, seem to have very little influence on the mode in which they are practically applied to sanitary work.

I would quote some words of a former president, Dr. B. G. Babington, which are not inappropriate:—"In the infancy of geology, first studied in this country, many phenomena observed in the arrangement of the earth's crust, as it is found on this island, were supposed to furnish fixed laws, and thus gave rise among our philosophers to divers ingenious generalisations. But when these same philosophers had, from the establishment of universal peace, the opportunity of taking a wider range and of studying the earth's structure, not in this country alone, but over the whole surface of the globe, they discovered in many instances that what they had supposed to be general laws were after all only exceptional cases. We require, therefore, in a study of epidemic diseases, as of geology, a wide field, in order that we may found theories on a sufficiently broad basis to avoid the risk of coming to partial and erroneous conclusions."³

On the question of importation of cholera in the Punjab epidemic (1875-6) and its spread by human agency, Dr. Bryden, in his report, p. 308 (1876, published 1878), says:—"The assertion amounts to this. The Punjab is divided into thirty-two districts. Cholera was introduced into seventeen of these in 1875, and therefore it spread. Cholera was *not* introduced into fifteen districts, and therefore did not appear, or, if it was introduced, influences of which we know nothing stopped its propagation in these districts. Unknown causes prevailing in the one half of the area are presumed to influence the human system so that it is capable of receiving cholera, and in the other to act universally as an antidote even to a cholera poison when introduced. This is what is offered as antagonistic to the theory which asserts that cholera is air-borne and is as far-flying as are the limits of natural areas. Primarily, these theories cannot

³ Address to Epidemiological Society, by Dr. B. G. Babington, December, 1850.

be reconciled ; the one or the other is false. If cholera is spread only by the human being, the theory which shows it to be air-conveyed is untrue ; if cholera is spread solely as an aerial miasm, then the theory which recognises only the effects of human intercourse is unfounded. The most that is admitted by the advocates of the human theory is, that the subjection of cholera to meteorological agencies is absolute, and that these influences can, and do operate so as to do away altogether with the effects of the poison, although imported. They do not recognise the entity *minus* the human being. The antagonistic theory, while holding, as opposed to demonstrable fact, the statement that cholera moves only by human agency, may, if necessary, be extended in its scope so as to embrace the other ; that is to say, the inquiry is left open as to whether or not the cholera entity, after being aerially distributed, may be subsequently propagated or spread by man. In the theory which connects cholera solely with man there is no such extensibility. Human intercourse must explain every fact of spread and propagation, and nothing is left to the play of natural agencies. Human intercourse, giving the widest scope to the signification of the term, cannot pretend to account for any fundamental phenomenon displayed during the progress of epidemic cholera ; and, therefore, I assert the theory to be radically untrue as applied to the behaviour of cholera in India. I do not say that the above statement will hold true all over the world ; and, even as applied to India, the theory does not preclude the possibility that cholera may be conveyed by the human being. Naturally the recoil is to the opposite extreme. The bold statement that cholera is never spread over an area unless human agency intervenes, is apt to be met by the equally dogmatic statement that cholera is as pure a miasm as malaria, and as little amenable to the control of man."

I would, in passing, just remark on some curious and interesting facts regarding the introduction of epidemic disease into certain localities where the exact conditions of the people and the visitors were known. The nature of the miasm or influence is altogether unknown, but it can hardly be doubted that something was imported. Doubtless, as science progresses, epidemiology will give a simple and rational explanation of such things, and it seems to me probable that the diffusion of epidemic diseases may have to be explained on some law of which the following instances are only varieties of expression.

St. Kilda is one of the outer Hebrides, a small island N.W. of Scotland. In a work on this island by Mr. George Seton, M.A., Oxon., mention is made of a peculiar form of epidemic influenza known as the "boat cough" or "stranger's cough," having all the symptoms of influenza. By this the natives are almost always attacked shortly after the arrival of a vessel from the outer Hebrides. They allege that the disease is most severe when the visitors come from Harris, and that they suffer less when the vessel hails from Glasgow or the distant ports. The malady first attacks those persons who have come most closely into contact with the strangers, and then extends itself over the whole community. By some it is attributed to the inhabitants exposing themselves to cold by rushing into the water to assist the strangers in landing, but this is not a probable explanation. Others allege that it is due to easterly winds, but the ships generally arrive with a westerly wind. By some it has been remarked that the cough is a yearly epidemic, but Mr. Morgan assures us that three

outbreaks have occurred in eight weeks, each following the arrival of a boat or vessel. It has been suggested that the sudden contact of a people, under exceptional circumstances as regards food and occupation, with strangers, is the cause which seems to exercise an infectious influence on them, though what the nature of it is no one knows.

Again, on the river Amazon, speaking of the gradual exhaustion of certain tribes friendly to the whites who inhabit the country near Ega, Mr. Bates says: "The principal cause of their decay in numbers seems to be a disease which always appears amongst them when a village is visited by people from the civilised settlements—a slow fever accompanied by the symptoms of a common cold, *de fluxo*, as the Brazilians term it, ending in consumption. The disorder has been known to break out when the visitors were entirely free from it, the simple contact of civilised men in some mysterious way being sufficient to create it."

A still more recent, as well as a more strictly parallel, illustration of the occurrence of the malady in question in another part of the globe is contained in the account of the cruise of H.M.S. *Galatea*, in 1867-68, where the following statement occurs:—"Tristan d'Acunha is a remarkably healthy island; but it is a singular fact that any vessel touching there from St. Helena invariably brings with it a disease resembling influenza. St. Kilda, off the west coast of Scotland, is known to be similarly affected whenever a party lands amongst the people from any vessel. Whatever may be the real cause of the mysterious ailment—whether it is produced by contagion, like certain other epidemic diseases, or by a feverish excitement arising from a contact of a higher with a lower civilisation—the actual occurrence of the distemper seems to be fully established; and the experiences of Ega and Tristan d'Acunha afford interesting illustrations of somewhat similar results in many different parts of the world."

Does not this seem to point to some subtle influence, the nature of which is unknown as yet? May not this in some measure account for the rapid decrease of the population of the South Sea Islands after contact with Europeans; and is it not pregnant with suggestive ideas as to the nature and the cause of transmission and diffusion of other epidemic diseases?

To return to cholera. In the present state of our knowledge we can only be guided by the inferences from well-ascertained facts and such laws as we have ascertained to be in constant operation, watching and carefully observing until we may, were it only by a process of exclusion, arrive at some deeper knowledge still. "So far," says the chief sanitary authority in India, "the history of cholera is full of enigmas and seeming contradictions; and though we have of late years collected many valuable data, and understood the importance of studying them on a broad basis, we know no more of the exact cause of the disease than our grandfathers did. We know that, whatever the cause may be, it flourishes in the midst of insanitary conditions of dirt and overcrowding, and especially of impure water, impure from whatever cause; we know that it is liable to occur under certain conditions, and at certain times and seasons, and we should endeavour to extend that knowledge, and hope, as we do so, to arrive at the precise nature of the disease itself." But it may be well to remember that "expenditure of public money must take place only on

observed facts and experience," not in accordance with theories. "It would be prejudicial to real sanitary work if opinions which have been promulgated in some parts of India, as to the cause of cholera being due *solely* to the state of the water-supply, were to take root."

It is most important that we should arrive at some definite conclusion as to the real nature of the disease, for it is impossible but that our conceptions on this subject must influence the sanitary measures that deal with it, and I believe the question must find its final solution in India, where the disease is always present in its endemic and seldom absent from the epidemic areas. The highest authorities—for both I have the greatest respect—differ *toto cœlo* on this subject. Is it that they are both right, though seemingly so opposed in their views? Like the knights who fought about the silver and golden sides of the shield, will they not change places and find why they differed?

I would venture to suggest that in India the inquiry might be pushed with more detail in regard to individual cases and outbreaks in certain limited areas; and that, on the other hand, one or more epidemiologists of European fame should be deputed to visit India and study cholera with the eminent men who have devoted so many years of close attention to it there. It might be, I believe it would be, that mutually they would gain from each other, and that those who went out would find their own views confirmed as to the disease in Europe—modified as to what they deemed it to be in India. Of one thing I am convinced, that simple truth is the object of their search; and I feel sure that from such combined action the greatest benefit would result.

I must now bring these remarks to a conclusion. I have made them purposely of a very general character, as I wished rather to indicate some of the subjects that we may profitably discuss, than deal with them in a controversial manner. I should like to have spoken of other epidemics—fevers, small-pox, the exanthemata and other miasmatic or zymotic diseases; of diarrhœa, dysentery, influenza, diphtheria, which all interest us as epidemiologists, and of those peculiar indigenous and endemic diseases such as leprosy, elephantiasis, beri-beri, and a host of cachetic conditions generally attributed to malaria, but perhaps susceptible of other explanations; of the interesting discoveries of Lewis, Bancroft, Manson, and others which have revealed to us the existence of parasites infecting the blood and tissues of men and animals to an extent hitherto undreamt of, probably having important causal relations with some of the endemic diseases hitherto referred to other causes, and on which I have already had the honour of addressing you. But time does not permit me to do so, for I have already detained you too long, and I can merely refer to them as subjects that I hope may from time to time occupy the attention of the Society. It only remains for me to thank you for the patient attention with which you have listened to me for so long.

